Eur J Vasc Endovasc Surg **16**, 543–545 (1998)

Correspondence

Ruptured Aortic Aneurysms

Sir,

I read with interest the retrospective study of van Dongen *et al.*[1] and agree that it is important to study the factors associated with mortality following surgery for ruptured abdominal aortic aneurysm (RAAA). The use of multiple logistic regression is a valid test to predict an outcome variable from the value of other binary variables.[2] However, the use of relative risk as a quantitative assessment of mortality risk (Tables 4 and 6) is flawed, as this is not a prospective study. Odds ratio is the preferred calculation for a retrospective study because of the way subjects are sampled.[3] If odds ratio is interpreted as a relative risk it will always overstate any effect size. This will be particularly exaggerated when the initial risk is high, as is the case for mortality following RAAA (overall hospital mortality in this study = 25%).[4]

E. P. L. Turton
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Authors' reply

We appreciate the reaction of Turton to our article. The main question is whether it is acceptable to use relative risks as shown in Tables 4 and 6 in our retrospective study on risk factors for mortality in patients with ruptured abdominal aortic aneurysm. We think the answer is yes for the following reasons. Firstly, the relative risks as presented in Table 6 are based on Cox regression analysis which is the appropriate way to analyse a follow-up study irrespective of whether it is prospective or retrospective. Secondly, for the relative risks as presented in Table 4 we used logistic regression which, in principle, estimates odds ratios. Turton states correctly that the use of odds ratios overestimates the relative risk in case of a high frequency of the outcome as recently reviewed by Davies *et al.*[1] Davies *et al.* demonstrated that odds ratios always overestimate relative risk but only when the risk in one of the comparison groups is higher than 20% will the use of an odds ratio lead to dramatic overestimations of relative risk. However, in our analysis for Table 4 the frequency of early mortality was 26 out of 309 patients (8.4%) and not the 25% hospital mortality as stated by Turton. Therefore, the odds ratios we estimated and which were presented as relative risks in Table 4 are, in our opinion, acceptable presentations of the association between risk factors and mortality.

A. de Boer, H. P. A. van Dongen, J. A. Leusink and F. L. Moll
Utrecht, The Netherlands.

Reference

1 Davies HTO, Crombie IK, Tavakoli M When can odds ratios mislead? *BMJ* 1998; **316** 898–991

Cognitive Testing

Sir,

We were interested to read the review of cognitive testing in patients undergiong carotid endarterectomy by Mr Irvine and colleages.[1] This paper is highly critical of the methodology employed by many previous studies. Some of these criticisms regarding the

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variation in cognitive testing, lack of control groups and long-term follow-up are valid. However, there are several points with which we take issue and we would appreciate the opportunity to put these to the authors.

The authors place a misleading emphasis on the question of whether cognitive function improves after CEA or deteriorates *per se*. The effect of CEA on cognitive function is unlikely to be constant because of all the variables associated with the operation and cognitive testing. In common with others, our study found that postoperatively some patients improved, but the majority stayed the same and some patients deteriorated.[2] Our interest as surgeons was in determining whether there was an underlying cause for deterioration and whether this could be prevented. The fact that some patients improve after CEA is interesting but ultimately academic. One could never envisage a situation where improvement in cognitive function became an indication for undergoing CEA. The only indication for CEA is the prevention of stroke and death and is likely to remain so.

In our own study of cognitive function, quoted in the review, we investigated a range of clinical outcomes, including cognitive function, in 100 consecutive patients undergoing CEA and related these to the incidence of intraoperative emboli detected by transcranial Doppler (TCD) monitoring.[2] This study found an association between greater than 10 particulate emboli detected during the dissection phase of CEA and a deterioration in cognitive function in the early postoperative period in seven out of eight patients. On several occasions Irvine and colleages misrepresent these findings in eight patients to suggest that we claim that CEA *per se* adversely affects cognitive function.[1] This is incorrect. Our hypothesis was that excess particulate embolisation during CEA was associated with reduced cognitive function scores and not the performance of the operation itself. We are soon to report a more detailed investigation of cognitive function and TCD detected embolisation incorporaating long-term follow-up and control groups which should clarify the relationship between intraoperative particulate embolisation and postoperative cognitive function.

The authors make certain recommendations regarding statistics. They suggest that the *t*-test is not appropriate as a device for analysing data, whereas analysis of variance is, and analysis of covariance is even better. The important point here is that the most appropriate test should be used when analysing data and this may be a *t*-test or it may be a more complex test. There is no *a priori* reason to believe that ANCOVA is better than ANOVA, which is better than a *t*-test.[3] The point about obtaining specialist statistical advice before starting any study is well made.

The authors suggest that cognitive tests should be chosen that are 'resistant to practice effects'. Despite claims to the contrary, such tests probably do not exist. You cannot prevent learning and learning leads to improved performance. They also claim that practice effects are minimised after 3 months. This is also a dubious claim. There is a large literature which demonstrates that the effects of a single exposure to a stimulus can last longer than this, indeed one study found improved performance up to 17 months later.[4] We would suggest that it is important to use parallel versions of tests and to use tests that include normative data for second and third administration. When this is combined with a suitable control group it will allow us to exert some control over variables such as practice effects, anxiety and the effects of surgery and anaesthetic.[5]

Finally, the article is packed with recommendations culled from the literature but low on specifics and the lack of recommendations based on their own experience (no work from the authors is quoted) weakens the credibility of the article. One can only hope that having performed their literature review and decided that they need to "stratify patients according to symptoms, correct for age-related changes, screen for depression, anxiety and dementia, use only cognitive tests which eliminate practice effects, account for lateralisation and have high test–retest reliability, standardise testing and retesting intervals, recruit control groups covering cerebrovascular, peripheral vascular and general surgery, incorporating surgical and non-surgical patients, correct for type 1 error and[2] 'the family factor' and finally undertake rigorous analysis of co-variance" they were not totally discouraged from either performing or publishing their own work on the subject.

Cognitive function is no different from other fields of research in which there are many confounding factors which one attempts to minimise but can seldom eliminate altogether. Despite this, it remains a valid field of research for the vascular surgeon because the human brain does so much more than just move the arms and legs about. Researchers should not be discouraged from performing pilot studies to identify potentially promising areas of research before embarking on more major studies investigating the large number of patients that adherence to these guidelines require.

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Leicester, U.K.

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Author's reply

Mr Gaunt and colleagues' lengthy correspondence raises some interesting points about the value of cognitive testing in patients undergoing carotid endarterectomy (CEA). Lack of control groups, the presentation and statistical handling of data, practice effects, and lastly the role of a review article are all at issue.

There is no suggestion that changes in cognitive function are an indication for performing CEA. The historically reported cognitive improvement following CEA is now recognised for what it was – a spurious effect of practice.[1] Mr Gaunt and colleagues have misunderstood the reason for including these studies in the review – they highlight the perils of not using control groups. The uncontrolled work of Gaunt *et al.* has stimulated renewed interest in cognition and CEA.[2] Their work tested patients early after CEA and found a deterioration in cognition in over one-third of patients (38/91). An association between microembolisation at surgery and cognition was drawn. The data concerning other patients' postoperative scores was not presented. Structuring of the review necessitated simple classification of the results of published work with respect to control groups and the effect of CEA on cognition. The review has focused on the available data presented in the paper by Gaunt *et al.* These results are not misquoted and the numbers of patients involved is given in Table 3 in the review.[1]

The statistical handling of cognitive data is of great importance, as confounding variables exist all too often.[3,4] The detailed arguments for the presentation of statistical analyses to support the reproducibility of cognitive function data is outside of the scope of the review and, I suspect, of little interest to the readership. Simple *t*-test analysis of controlled complex multifactorial cognitive data may be misleading and expert advice should be sought. No test eliminates practice. Tests with parallel forms which are resistant to practice do exist.[5] These tests are not yet validated in an aged population but provide us with an avenue for future study.

What is the purpose of a review article? Mr Gaunt and colleagues question the number and detail of the recommendations made in the review. Subject review without comment on the success, errors and limitations of previous work amounts to publication for publication's sake. The recommendations given in the review are extensive, but they deal with issues that are of great importance in avoiding incorrect assumptions. It is of note that initial peer review of this article suggested that not enough recommendations were made! Perhaps the fact that Mr Gaunt and colleagues are to utilise "... parallel versions of tests and to use normative data for the second and third administration ... combined with a suitable control group" for their further work means that some of the recommendations that are outlined in the review will be adopted by others.

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